

PROCEEDINGS  
OF  
THE ROYAL  
MEDICAL AND CHIRURGICAL SOCIETY  
OF LONDON.

*Tuesday, January 25th, 1898.*

W. HOWSHIP DICKINSON, M.D., President, in the Chair.

NORMAN MOORE, M.D.,  
ROBERT WILLIAM PARKER, } Hon. Secs.

Present—29 Fellows and 3 visitors.

The minutes of the last meeting were read and signed.

The following recently elected Fellows signed the Obligation, and were admitted by the President:

A. P. Beddard, M.B., B.C.

G. F. Blacker, M.D., F.R.C.S.

W. Gladstone Clark, L.R.C.P., M.R.C.S.

Walter Stacy Colman, M.D., F.R.C.P.

F. Woodcock Goodbody, M.D., B.Ch.

Robert Hutchison, M.D., M.R.C.P.

The following gifts were announced, and votes of thanks were awarded to the donors:

On Cardiac Failure and its Treatment, with especial reference to the use of Baths and Exercises; by Alexander Morison, M.D. (1897): presented by the Author. Transactions of the Glasgow Medico-Chirurgical Society, vol. i, Sessions 1895-6, 1896-7; edited by C. O. Hawthorne, M.B. (1897): presented by the Medico-Chirurgical Society of Glasgow. The Latest Fruit is the Ripest; by F. J. Gant, F.R.C.S.; presented by the Author. Le traitement spécifique du paludisme d'après la biologie de l'hématozoaire; par F. Lapasset (1897): presented by the Author. L'assistance maritime des enfants et les hôpitaux marins; par Charles Leroux (1892): presented by Dr. F. Parkes Weber. The Year-Book of Treatment for 1898: presented by Messrs. Cassell & Co., Limited. The Origin of Zymotic Disease; by F. A. Cooper, B.Sc.Lond.: presented by the Author.

The following paper was read :

Immunity and Latency after Operations for Reputed  
Cancer of the Breast : by A. MARMADUKE SHEILD.

(*Abstract.*)

THE paper commences by a short sketch of the general results of the old or incomplete operation for cancer of the breast in this country during the present century. Attention is drawn to the fact that the more extensive operation as now advised was promulgated by Moore as long ago as 1867. Almost every surgeon of experience was able to quote notable exceptions to the generally received rule that rapid recurrence was invariable. An investigation into the subject leads to the belief that the results of the old or incomplete operation were not as universally bad as generally stated. A number of cases have been collated from various sources to show this. A much larger number could have been quoted, but the cases are collected rather with the view of illustrating the subject, and of evoking comment and discussion, than for the sake of compiling a very large number. The source of information is quoted in the tables, and the reasons for believing the cases to be genuine are carefully stated, especial stress being laid upon the opinions of well-known surgeons. In the first group, 44 cases are recorded where long immunity resulted after operation with no recurrence. In the same group are 64 cases, where recurrences local and metastatic took place. Here it is notable that some of the cases, where life has been longest preserved, have been those marked by local recurrences, which have been removed one or more times. In some of the cases return of the disease took place at remarkably long periods after operation. The paper concludes by discussing some of the various groups which these cases form, and it is argued, that some of the best results obtained must be rather due to some peculiarity in the growth or the individual, than the actual nature of the operation. The late recurrences observed make no doubt

that the adoption of the rule of Volkmann of a three years limit is too absolute, and the period of time too short for the pronouncement of "cure." A collective investigation of the true results of the modern extensive operations is much to be desired.

MR. PRIDGIN TEALE.—The author wrote to me for details of any cases of my own bearing on the subject of his paper, and owing to the method of keeping notes of my private operations I have a fairly complete record for the last thirty-five years. On looking through these I find a good account of the subsequent history of these cases. I always noted whether there was any evidence of return, and if I heard some years later that patients were still alive the fact was noted. Evidence of this kind can, I think, only be efficiently obtained from private practice. Of course there is the objection that private notes are not open to control and criticism, but must be taken on the authority of the observer. I contributed in all some eighteen cases to the author, and with one exception none had died within eight years. I must ask you to believe that my diagnosis as to their being cases of scirrhus of the breast was correct. It was essentially a clinical diagnosis, and in those early days, though we had not the means of careful microscopical observation which we now possess, we used the microscope in all our cases very carefully; and on the whole I think the great majority of the cases may safely be looked upon as genuine scirrhus. Some confirmation of the diagnosis was afforded in most of them, in that all except two or three were ultimately followed by death from disease of the nature of scirrhus. That leads me, by parenthesis, to utter a protest against the very mischievous doctrine that a case which remains free for three years after operation is cured. We ought, I think, to object to the use of the word "cure" at all. It is liable to be misleading and harmful, and leaves the door open to a certain amount of deception. I was surprised, on looking over my cases, to find how many there were—I cannot give the proportion—who lived for a very considerable time. There were many more than I had anticipated. Mine was the old-fashioned operation, with this difference: when I was commencing practice as a surgeon the plan of cutting out the cancerous tumour was practically being abandoned, and the rule was being established that the whole breast must be removed, so that from the beginning my operations were done on that basis. With regard to axillary glands, in those days if we could not feel any glands we did not open the axilla. Of late years, however, I have always opened the axilla in order to ascertain the presence or otherwise of any enlarged glands, my rule being

to remove every gland I could detect. So far, then, as any enlargement of the glands was concerned, you may take it that they were for the time being thoroughly removed. I did not, however, dissect the contents of the axilla, as is now apparently the prevailing doctrine. In the light of this modern doctrine I am surprised to find that my neglecting these precautions did not seem materially to have affected recurrence. I mention this as a matter of observation. It is possible that what is done now may prove to be the right thing after all; still the old operation was often a very satisfactory operation. It is striking to hear how the complete operation, as at present performed, is sometimes followed by early recurrence. In my own experience return of cancer internally after cancer of the breast has been comparatively rare, certainly nothing like the proportion reported here by Halsted. The facts which Mr. Sheild has brought out are very important in this respect, now that the question as to which is the best operation is being acutely discussed. No one system should rule us, but we must choose the one best suited for the requirements and circumstances of the individual case. If this extensive operation had been done twenty or thirty years ago I am sure we should have had a much larger number of fatal cases after operation. They may be comparatively safe now that circumstances have changed, for antiseptic surgery has given us a more free hand. I am told that some operators go so far as to remove the pectoral muscle as a whole almost as a matter of routine, but this I imagine must often entail disability of the arm. I have once or twice removed the greater part myself. Whether the mortality is unduly increased by these extensive operations, or whether practical inconvenience remains, time will tell.

MR. BRYANT.—This is a very valuable paper, because it brings together the experience of many still practising surgeons. If we cannot draw from these cases any very definite opinion with respect to the development or treatment of cancer of the breast, these cases must help us by enabling us to place a more favourable view, so far as prognosis is concerned, before our patients. To be enabled to tell patients that a reasonable proportion of cases may live for from five to twenty years and upwards is a fact which must be encouraging to those who submit themselves to an operation generally with a mind rather disturbed by their future prospects, for we know too well that most people who have been told that they are the subjects of cancer entertain very hopeless views as to their future. I have contributed a few cases to the author's list, and doubtless could have furnished very many more had I had more time, but still they must be looked upon as exceptional cases. For my own part I may say that in the cases I have had opportunities of watching and tracing to the end I have deduced some con-



clusions which appear worth stating. I found that out of seventy-two cases in private practice which I followed to the end, although about half sank within three years, the other half lived from five to ten years, and Mr. Sheild's paper points out how patients may live even for twenty years. These are facts which must help us very much in our clinical work. With respect to the operation, I do not like the terms "complete" and "incomplete," for if we can show that the so-called incomplete operation is the better of the two it is the proper treatment to pursue. The word "incomplete" rather suggests that the surgeon ought to have done something which he has omitted. I must admit that the average operation which I perform is the so-called incomplete one, for it has never been my habit in all cases to clear out the axilla and to take away all the connective tissue right down to the floor of the axilla; and I am not at all prepared to say that my results are worse than those that I know of where the so-called complete operation has been carried out. I take away everything I can see or feel to be diseased; I always cut into the axilla to explore it, but I do not dissect it in every instance. I agree with Mr. Teale that even where recurrence has taken place after the operation it has rarely been in the direction of the axilla or in glands, but it has generally been connected with the skin and subcutaneous tissue, possibly just below the axilla or in the scar itself. I have always found a kind of consolation when I have not taken away the glands to find that the recurrence has occurred, not in the line of the lymphatics but elsewhere, so I am not disposed to agree with those who assert that under all circumstances what the author calls the complete operation should be performed. I believe that the results of his incomplete operation are as satisfactory as those of the complete. In operations undertaken on women fairly advanced in years, as Mr. Teale has pointed out, we now can and do operate with comparative safety in a way that was impossible twenty-five years ago, in consequence of the antiseptic treatment of wounds. I think, especially in hospital practice, where one has an opportunity of watching the practice of others, the routine method of performing the "complete" operation has added to the mortality of operations on the breast. I may say that I have never yet removed the whole pectoral muscle, and I do not think I am ever likely to. A case which requires such extensive measures is probably one that would be better left alone. I have often removed the anterior surface of the muscle with the fascia, fairly extensively even, but I have never divided it wholly and taken it away, nor am I convinced that such is the right thing to do. With all respect to the author, deviating a little from what he has said in respect of the natural history of cancer, I should like to note that cancer is not always a progressive

disease. It has a period of growth, and often a period of retrogression. We are all familiar with carcinoma fibrosum, which goes on for many years, and may really appear of little importance to the patient. I have instances in my mind of cases of this kind of fifteen to twenty years' duration, and it was all that time really only a local disease. I have also seen instances where this carcinoma fibrosum has been going on for many years, and has ultimately been removed, and I have been struck by the fact that removal of this until then local disease has excited a recrudescence, as it were, of the cancerous growth, and within a few weeks or months death has taken place from disseminated cancer, in which you find scattered over the body carcinomatous tubercles, and the patient soon dies; so much so that my own feeling is that many of these cases of very chronic carcinoma are best left alone, and that it is a mistake to interfere with them. I may mention an odd instance of cancer of one breast associated with sarcoma of the other—that of a lady whom I was asked to see with sarcoma of the right breast which contained a large tumour the size of a cocoanut. It was undoubtedly sarcomatous, and the microscope confirmed, after its removal, the clinical diagnosis. On examining the opposite breast I found that it was the site of a carcinoma fibrosum, which was contracted up—breast, nipple, skin and all,—but there were no enlarged axillary glands. The sarcoma, however, was growing fast, and I removed it. She was sixty-five years of age, and in the course of the next five years I removed recurrent growths of sarcoma at least eighteen times. During the whole of this period the carcinoma of the left breast remained absolutely quiescent, and it was only during the last few months of her life that it began to develop, and death ensued. Then there is the fact that with these carcinomata we often find little skin tubercles connected with the disease which come and go. I remember an instance of that in connection with carcinoma fibrosum, in which the patient had tubercles scattered all over the side, which used to wither and go, leaving natural skin behind, or in some cases white cicatrices. I have often seen this since then, and in a recent case I made a sketch so as to be quite certain as to their distribution. I have now in my hand such a sketch, taken from a lady aged fifty-seven who in 1893 had the breast and glands removed. Three years later a small lump appeared just below the scar, which I removed. I saw her again later on, in 1896, when these tubercles had existed for at least three months. In March, 1897, the tubercles had all gone, nine months after their first appearance; and when I saw her again, a month ago, she was quite well. This disappearance of the secondary deposits of cancer is a fact worthy of recollection.

MR. CHRISTOPHER HEATH.—I wish to emphasise the protest

against Volkmann's doctrine as to three years' freedom from recurrence implying cure of the disease. It is in my opinion a fallacious doctrine, and I regret that an eminent surgeon should have brought this statement prominently forward in his lectures, and should have appended to cases which had not recurred for three years the note "cured." Among the cases comprised in the paper there is one in which I removed the breast for undoubted cancer in 1883, and the patient is now quite well. I wrote to several medical men about other cases, and I found that a lady on whom I operated in 1886 is well at the present time; one whose breast I removed in 1891 is still in good health; another in 1893, whose mother by a strange coincidence had her breast removed for cancer by another surgeon a few months later, thus showing a strongly marked family tendency, is perfectly well. I saw her early this year, and made a careful examination of the scar, and there was not the slightest symptom of disease, and her mother also is quite well. To come back to a case seen with Mr. Marshall in 1887, I saw the patient for fully three years and I put her down in my mind as cured; but in 1892 I saw her death in the papers, and I took some trouble to find out what she died of, and I found it was mediastinal disease of a cancerous nature. In answer to one of my inquiries in respect of a case where I operated in 1890, I heard this morning that she only died in 1895, more than five years afterwards, from cancer of the lung on the same side. There was a case which at three or even four years would have been pronounced cured, yet she died of cancerous disease. I was surgeon to the Hospital for Women in Soho from 1870 to 1874, and I had a large number of operations for cancer of the breast. I find on referring to a lecture of mine which was published in the 'Lancet' for 1871, that I spoke of its being most important to remove all skin, fat, and cellular tissue which might possibly contain cancerous cells; and if the growth were adherent to the pectoral muscle I urged that there should be no hesitation in removing it, if necessary to some depth, adding that any enlarged glands in the axilla should be removed at the same time. This has been my practice since 1870, so that modern ideas, and particularly Halsted's unnecessarily severe operation, are really little more than what many surgeons have done for the last twenty-five years. I think the results show that we have in a great many cases satisfactorily removed the disease. Of course there have been recurrences, sometimes *in situ*, but more frequently in the axilla, and sometimes in other parts of the body. Efforts have been made at different times to relieve recurrent cancer by other remedies than the knife—Chian turpentine, for instance, and caustics. There is another form of treatment still in vogue, and that is the electrical treatment of cancer. I have had some little



experience of this; that is to say, patients for whom I had said I could do nothing more have told me that they intended to put themselves under the care of the gentleman who professes to arrest cancerous disease—he does not go the length of promising to cure it—by powerful intermittent electrical discharges. All I can say is that I have never seen the slightest benefit from it; the disease has always followed its usual course, and has invariably ended fatally. I think it right just to say how much indebted we are to the foundation for cancerous patients in the Middlesex Hospital, where these unhappy patients are taken in and cared for until they die. That is of great benefit not merely to the patients but also to science. Medical men often say they cannot find out whether their patients are alive or dead. Now there is no difficulty in the matter, for one has but to go to Somerset House and look over the admirably kept records of the Registrar-General, and if one only knows the name and approximately the date of the death, it does not take long to ascertain whether the name is there and to see the certificate of the cause of death. You can search five years' records for one shilling, and if necessary you can take a second shilling's-worth.

Mr. BARWELL.—I also should like to protest against the term “cured.” I have had extensive experience in various cases of cancer. Of course I have had cases in which recurrence has taken place within three years, sometimes a good deal within. I have had two cases of cancer, one of the breast and one of the tongue, which were operated upon twenty years ago and are still living. One other case is alive now, fifteen years after the operation. Six of my patients have died of other diseases without recurrence, all operated upon more than ten years previously to their death. I have put down five cases in which there has been a long immunity, one an unmarried lady operated upon in 1886. She was perfectly well for seven years and six months, and in 1893 I found two small tumours in the costal cartilages, mere nodules, but very hard, and these I removed and she was still well years later. Another case operated upon in February, 1881, only died of recurrence in May, 1896, fifteen years after the operation. One lady upon whom I hesitated to operate at all, but in whom I cleared out the axilla and removed much of the pectoral muscle, remained well until 1894, when recurrence took place in the pectoral on the same side at the outer edge of the scar. Another operated upon in June, 1884, came back with small nodules in the scalp in February, 1889. These were too numerous for operation, and I lost sight of her. In a case of cancer of the tongue, where I always operate by my supra-hyoid method, which I still prefer to the scissors operation, I have a considerable number of cases without recurrence for from seven to twelve years, and one on which I operated sixteen years ago is still alive. To speak therefore of cure after so short a period as



that mentioned in the paper is absurd. In the matter of opening the axilla when there is no external sign of disease therein I think we ought to be guided by the site where the tumour occurs. In my experience if the tumour is on the inner side of the mamma, near the sternum, the occurrence of swollen glands in the axilla is much more protracted, and one may largely trust to the fact that there is probably little to remove. Nevertheless I consider it wise in a case of cancer of the breast to cut through the fascia of the axilla and to pass one's finger in, the better to search for any enlarged glands and also for hardened cords. It must be remembered that we have not only to do with glands but with the lymphatic vessels themselves, which are often hard and cord-like. I also consider it essential to remove all the fascia covering the pectoralis, to clear it away with great care; especially is it important to examine that muscle very carefully, removing all whitened or hardened spots, even if in so doing one is obliged to go right down to the ribs and even shave away some of the cartilage from which the muscle arises. Personally I am not at all enamoured of what the author calls the incomplete operation. I do not mean to say that in all cases we should make a clean sweep of everything. There should be a certain amount of eclecticism, but where there is extensive rapidly growing disease I believe it to be essential to make the operation as wide as possible with regard to the deeper parts. With regard to the skin I believe one is very much freer from any chance of recurrence if one can get the wound to heal by first intention, for which it is necessary that there should be very little tension. In removing a great quantity of skin one cannot of course get this, and once, after a certain number of days, I resorted to transplantation of skin in order to get the wound to heal as quickly as possible. We must, however, be careful in other directions; first that anything like diseased skin must be removed, and secondly not to remove more skin than is necessary, so that we may get as rapid healing with as little local irritation as possible.

Mr. BUTLIN.—I had intended, sir, to speak on the pathological points raised by Mr. Sheild's interesting paper, but the discussion has taken so decided a clinical turn, that I feel it would be out of place to do so. I shall therefore direct my remarks to two of the questions which have been discussed to-night by those who have preceded me. In the first place I am afraid that Mr. Sheild's paper may produce a different effect from that which he probably desires. Seeing what excellent results have followed comparatively small operations in so many cases, surgeons may be tempted to perform these operations rather than those which are directed to the complete local removal of the disease. For the last eighteen months or two years I have been performing Halsted's operation in almost

all cases, not because I believe that no other operation is likely to be successful, but because I should like to see whether such an operation may not furnish a better percentage of good results than have hitherto been obtained by what may be termed the English complete operation. It certainly permits the dissection of the axilla to be more safely and thoroughly accomplished than any other operation I have practised, and the movement of the arm after it is quite as good as after the operations which do not include the removal of the pectoral muscle. I am sorry to see so pessimistic a view of the operative treatment of cancer prevail in this room. "Once cancer, always cancer," seems to be the theme of the speakers. I should be still more sorry that this expression should go forth to the profession and the public. So far from objecting to Volkmann's claim of "cure" for every patient who is alive and free from disease more than three years after the last operation, I accept it heartily. I quite agree with Mr. Sheild that probably the time is rather too short for cancer of the breast; but if it is extended for the breast, it ought to be shortened for such parts of the body as the tongue, where recurrence and glandular affection are of much earlier date. By-and-by, with a better knowledge of the course of the various malignant diseases of different parts of the body, we may be able to fix, much better than we can do now, more correct time limits. Until then I am quite content to accept Volkmaun's three years' limit for all parts of the body. Even if this should lead to an optimistic view of the operative treatment of cancer, which is not justified by the results of operations, I believe that is a much better frame of mind for both patient and surgeon than the opposite view. What can be worse for patient and surgeon than the belief that there is no cure for a cancerous patient, and that an operation is merely palliative? What is more likely to lead to incomplete operations, and even to carelessness on the part of operators, than this "fatalist" theory of the incurability of cancer?

On the motion of Sir THOMAS SMITH, seconded by Mr. BARKER, the discussion was adjourned to the next meeting.

# P R O C E E D I N G S

OF

## THE ROYAL

### MEDICAL AND CHIRURGICAL SOCIETY

### O F L O N D O N .

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*Tuesday, February 8th, 1898.*

W. HOWSHIP DICKINSON, M.D., President, in the Chair.

NORMAN MOORE, M.D.,  
ROBERT WILLIAM PARKER, } Hon. Secs.

Present—43 Fellows and 7 visitors.

The minutes of the last meeting were read and signed.

A ballot was held for the election of candidates for the Fellowship; and the Scrutineers reported that the following gentlemen had been duly elected:

John Henry Bryant, M.D.Lond., M.R.C.P.Lond.  
 William Henry Corfield, M.D.Oxon., F.R.C.P.Lond.  
 Louis Albert Dunn, M.S.Lond., F.R.C.S.Eng.  
 Alfred Downing Fripp, M.S.Lond., F.R.C.S.Eng.  
 Edwin Hurry Fenwick, F.R.C.S.Eng.  
 John Alfred Gray, M.B.Lond., M.R.C.S.Eng.  
 Arthur Corrie Keep, M.D., C.M.Edin., M.R.C.S.Eng.  
 Arthur Carlyle Latham, M.B., Ch.B.Oxon, L.R.C.P.  
 Lond. and M.R.C.S.Eng.  
 John Bowring Lawford, F.R.C.S.Eng.  
 John McFadyean, M.B., C.M.Edin.  
 Herbert Murray Ramsay, F.R.C.S.Edin.  
 Leonard Rogers, M.B., B.S.Lond., F.R.C.S.Eng.  
 Alfred Salter, M.D.Lond.  
 John Lynn Thomas, F.R.C.S.Eng.

The following candidates for the Fellowship were proposed for election, and their proposal papers were ordered to be suspended in the Library:

John F. H. Broadbent, M.D., M.R.C.P., M.R.C.S.  
 H. Ronald Carter, M.R.C.S., L.R.C.P.  
 Andrew Stark Currie, M.D., M.R.C.S.  
 Percy James Edmunds, M.B., B.Sc., M.R.C.S., L.R.C.P.  
 Alexander Granville, L.R.C.P., M.R.C.S.  
 James Morrison, M.D.



The following gifts were announced, and votes of thanks were awarded to the donors :

Vaccination Commission: Reports on Outbreaks of Smallpox (1891-6); by Sidney Coupland, M.D.: presented by Dr. Coupland. Lives of Northern Worthies; by Hartley Coleridge; 3 vols. (1852): presented by Dr. Oswald A. Browne. Remarks on the Surgery of the Vas Deferens, relative to some Urinary Disorders; by Reginald Harrison, F.R.C.S. (1898): presented by the Author.

The discussion on Mr. Marmaduke Sheild's paper on "Immunity and Latency after Operations for Reputed Cancer of the Breast" was reopened by Sir THOMAS SMITH, and continued by Mr. JONATHAN HUTCHINSON, Mr. T. W. NUNN, Mr. HOWARD MARSH, and Mr. BARKER.

Sir THOMAS SMITH.—Mr. Marmaduke Sheild has very properly given us his cases without drawing any dogmatic conclusions, leaving us to form our own opinions. Until Mr. Butlin spoke I had imagined that the author was in favour of what is called the old or incomplete operation. So impartial has he been, that the impression his paper conveyed to me was that it had been written by some one who himself practised the incomplete operation, and had found it serviceable, and was bringing forward statistics to prove that it was a good operation. It is twenty-four years since a discussion on the subject of breast cancer took place; that discussion bore mainly on its pathological aspects, but to-night we are chiefly occupied in discussing its clinical bearings. The author states that the object of his paper is not to compare the old operation with the new, but to show (1) that the results of the old operation are not so universally bad as is generally estimated; (2) and to illustrate what he terms vagaries of cancer, and the extraordinary periods of latency of the disease in many instances. I think that if these results had been appended to a list of cases where the new operation had been performed, they would have been considered highly satisfactory by the advocates of that operation; but I beg to call attention to the fact that most or nearly all of these cases have been treated by the old or incomplete operation as it is called. Mr. Butlin being a strong advocate for the complete operation thought that the author had done harm to his cause by giving the cases he has brought forward, meaning, I suppose, that he had given too favorable an impression of the results of the old operation. His results, however, tally with my own experience of that operation. In contrast to Mr. Sheild, and I may say Mr. Watson Cheyne, I venture to notice the tone adopted by many who have written in favour of the new operation—a tone of dogmatic assertion and of ill-concealed pity for

those who have not yet adopted the complete operation. Having in view how little we know of the real cause of cancer, it becomes us to be cautious in forming conclusions, and to keep an open mind with regard to the subject, and be ready to receive any evidence that may be forthcoming, and above all to avoid dogmatism. It is striking to observe that we are now discussing the subject of breast cancer on the same lines and with the same objects as it was discussed at the Pathological Society twenty-four years ago; indeed, we have made little if any progress in our knowledge of the nature of the disease, and are still debating how to treat it. The new operation originated, I believe, in America, was elaborated in Germany, and was adopted in this country as it seems to me rather hastily, and advocated very warmly, and that before it had been put to the test of experience. I am quite willing to allow that the operation may be a good one, and that the assumptions on which it is founded may be true; but at present its superiority over the old operation is not yet proven. The basis and justification of the new operation seem to me to rest on the assumptions (1) that breast cancer is a local disease; (2) that it is curable by operation; (3) that after three years' immunity from the time of the operation it is cured. It is highly probable that breast cancer in its early stage is a local disease, but I cannot quite understand from reading books about this method of treatment, and by reading the histories of cases, at what period it ceases to be local. I suppose the most ardent advocate of the new operation will allow that at some stage cancer ceases to be local and becomes generalised, and the question is at what period of breast cancer does it become general? Some of those who advocate the operation seem to consider that it never becomes generalised. It must be some view of this kind that justifies them in performing such serious operations as they do perform. It must require a very robust and conscientious belief in the continued local nature of cancer to justify removal of the upper limb and shoulder-joint for its treatment. Then when does the disease cease to be local? I cannot say, but one must think that when glands beneath the clavicle are affected, when the arm has to be removed at the shoulder-joint, or the clavicle divided in order to get out these glands, the disease has ceased to be local. I would refer to another disease of local origin in which a poison is introduced, where we can fix the date of its introduction at a known point—I mean syphilis. A certain induration takes place, and when that is so we know that the patient has syphilis—a constitutional disease,—which at that period is generalised. I believe it is not a common practice to remove the foreskin or the whole penis to get rid of the disease. This operation has, I believe, never been repeated by those who have once given it a trial. Here we have to do with a disease

the generalisation of which we know takes place in a very short time. By the time we can say for certain that the disease has been communicated, the disease has already become generalised. In this sense it is, I think, exceedingly likely that breast cancer is a local disease, but it differs entirely from syphilis in that you cannot fix the period at which cancer begins. No one can say how long a time has elapsed between the deposit of cancer as a microscopic structure in the female breast, and its existence as a tangible tumour. That being so, one can only surmise that it may have been a long time, if the growth of cancer from a microscopic object to a tangible tumour is as slow as it sometimes is in the case of a tangible tumour. It is possible, indeed probable, that by the time the disease can be recognised it has ceased to be local.

The next postulate is that cancer is curable by operation. I will only say that it is curable by operation if you like to assume that if at the end of three years the patient is free from recurrence, then the patient is cured; but we know that cancer may return after three years. The author's cases afford proof that it is not cured merely because it has not recurred within three years, and I regret very much that the word cured has been used at all; I think it has had a bad effect on the profession and on the public. These tables are published, they are in print, they are seen by the victims of this disease, and a certain number of surgeons say they can cure cancer while there are others who cannot cure cancer, and it has come to this, that they are called cancer-curers, which in the present state of our knowledge is not an honourable title, though the time may come when a cancer-curer may be held in high repute. As regards my own experience I can confirm to a great extent what the author has said. It is noted both by Mr. Watson Cheyne and by the author that in many of these complete operations where there has been no return locally there has been a tendency to metastasis. In clinical lectures I have called attention to this tendency, and I have noticed that where there is a small local return there is often great prolongation of life. I cannot explain this, but my experience is that people who have no local returns very often live less long than those who have tubercles or small returns in the wound or in its neighbourhood. If this be the surgeon's fault, it seems to me to be very often to the advantage of the patient. I do not, however, think it is any good referring to the practice of more than, say, twenty years ago for the purpose of comparing the results with those of the present day. Prior to that date operations were done under such different and unfavorable circumstances, that many patients died; there were no means of compressing arteries, and consequently operations were much more hurried: so that although it is a common way to intro-



duce a paper with a reference to the bad results obtained by Brodie and others, I do not think it has any bearing on the subject. Sir James Paget has expressed a very depressing view about cancer, and I think he was justified in so doing; it may be that the type of the disease has altered, for people certainly live longer now after operation for breast cancer than they did thirty or forty years ago, and I am now within two years of half a century's experience of operating and seeing others operate on the breast. Mr. Sheild has demolished the contention that three years' immunity means a cure, showing that the disease may occur at any interval after the operation up to twenty-one years; indeed, within the last few weeks I have had to remove a cancerous gland from the axilla of a patient operated upon for breast cancer twenty-four years ago by Sir James Paget. Professor Gross, who early advocated the complete operation, estimated that only 2·3 per cent. of returns took place after the third year; but König, as quoted by Watson Cheyne, puts it at 15 per cent., so that there is no agreement as to the proportion of recurrences after the third year, but it must be obvious to all of us that a large number of cases recur after three years' immunity. I would urge that those who take a strong view one way or another about cancer of the breast and its operation should bear in mind certain possibilities. It is quite possible that the period at which cancer returns depends upon the date in the progress of the disease at which the operation was done, so that if a woman has already had it two years she is likely to live longer after operation than if she has had it four years. Yet in these tables all dates of disease are massed together, and cannot be distinguished. Then, too, the period of immunity must largely depend upon the type of growth and the soil in which it grows. In florid child-bearing women it is admitted that the disease runs a more rapid course than in thin, pallid, anæmic women. Another possibility which I hardly like to mention is that the removal of healthy lymphatic glands may conceivably favour the return of the disease. Mr. Teale hinted at this—that the removal of glands from the axilla did not improve the statistics; in fact, he implied that the removal of the glands had rather the opposite effect. We must remember that this is quite possible. I should like to refer to a case of Mr. Cheyne's, who is of the opposite way of thinking. He speaks of a case where he and Sir Joseph Lister removed a cancer on a certain date, the original operation having been done fifteen years previously. At that time a very few small hard glands were felt in the axilla, but were not touched, and they remained *in statu quo* until a short time before the second operation. They proved to be carcinomatous. I cannot help thinking these glands may have afforded a certain protection against the spread of the

disease. For my own part I remove all visible or tangible disease freely, whether in the breast or in the axilla, but on more than one occasion I have removed the disease and not the whole breast. Now in hospital practice it is impossible to trace cases, but I have looked up two private cases where I performed that operation with the following results. A patient of Mr. Thompson, of Sevenoaks, aged fifty-eight, had a cancer of the nipple and areola, the tumour being not much bigger than a large pea; this was freely removed, chloride of zinc being applied to the raw surface. She remained well five years, the disease then recurred in the axillary glands, and she has had three operations for local recurrences in the axilla. The breast remains quite healthy, and now at the end of seven years the patient is in good health, without visible disease. A patient of Dr. Brewer's with cancer of nipple and areola; ten years ago it was freely removed, and the wound allowed to granulate; at the present time she is in good health, and free from all signs of disease. These results are not so disastrous.

MR. HUTCHINSON.—My experience goes entirely with that of the author. I am a very firm believer in the operative cure of cancer, using the term cure, of course, in a loose sense; but we know what we mean by the term. I think the older surgeons did many cures as well as those in modern times. He has quoted one in illustration of that. On looking at my notes I find a note from the late Dr. Smith, of Cheltenham; that of a lady who was operated on at the Westminster Hospital by Phillips in 1849. The growth was examined microscopically, and was declared to be scirrhus. She lived until 1869, and then died of what was called keloid of the skin of the chest, no doubt a return of the cancer of the breast over the other breast, not over the one that was removed. Here we have an example of twenty years' interval. I may add that Mr. Smith mentioned to me another case in which the growth was removed by the same surgeon in 1849, and in 1870 the patient was still alive and well, and in this case also the microscopical examination showed it to be really a scirrhus. The last speaker has just told us that our pathology has not improved in respect of the nature of cancer. Now I think we have advanced a good deal in that we now regard cancer as after all allied to a chronic inflammatory process, that it is infective, and that it is a local disease for a certain length of time. I do not know that his comparison with syphilis goes for very much, but the analogy between a cancerous tumour and a focus of syphilitic infection is that we must find out how long it has existed as a local disease. If you cut out the infected spot immediately after infection, no doubt you would prevent its generalisation. Therefore we must find out in each case what is the period during which the infection is only local. As regards the local

and constitutional origin of cancer, we must keep in mind that there is a predisposition to the local origin. That predisposition continues to exist in future years when once it has appeared, and in many of the cases in which it is implied that this recurrence or development after very long periods after the first operation is proof of non-cure, the assumption is one in which I cannot concur. I think recurrence after long periods merely points to the patients having a constitutional tendency, and that they have developed another cancer, especially when it appears in distant parts, as for instance in the other breast. That is a very fair suggestion. Strong as I hold that cancers are during certain periods strictly local, I am equally strong in the belief that there exists a strong hereditary predisposition to cancer. I think, then, that our knowledge of the pathology of cancer as a local disease has grown. I think we owe much to Dr. Morgan, who was one of the first advocates of this view. One of the first medical papers I ever read was by him on the local origin of cancer, delivered at the Hunterian Society, and I pointed out that it was to early operation that we must look for any chance of mitigating the disease. I am an advocate for the very earliest operation possible. For a time I made it a rule to clear the axilla; but in common, I believe, with many others, I have relaxed that rule, and only meddle with the axilla if I can feel something there. I am not at all convinced of the correctness of the practice of making it an unswerving rule to clear the axilla. I operate for cancer, believing it to be the only chance of cure, especially if performed at an early stage; moreover, we cannot measure this stage. We cannot say how long this cancer is local, nor how soon it may have shed some material which will infect distant parts; but you are bound to give your patient the benefit of the doubt, and to remove it whenever removeable. Even if it has spread germs I would still take it away, because the patient is a great gainer by having a focus of disease taken away, even if you can no longer prevent its recurrence. I could quote a large number of cases which would go to prove the same results as those set forth in the paper. I am in ignorance on one point, viz. as to what is the "new" operation. I should have thought we had all long been in the habit of doing a free operation, and I do not see what is to be added. [Sir Thomas Smith.—It is the removal of the pectoral muscle and the contents of the axilla.] But the removal of the pectoral muscle is a very old thing. I remember when I was surgeon to the Metropolitan Free Hospital it was urged upon me. I should not think of removing it unless involved in the disease. As regards the selection of cases I may state as a matter of clinical experience that my best cases have not been those which seemed so at the time of first seeing them. One of my best cases, regarding the length of time that elapsed



after operation, was one in which the tumour had been growing very rapidly in a comparatively young woman. It had only been noticed a few months before, and was as large as a child's fist. It was reported to be a soft, rapidly growing mass of scirrhus. I removed the whole breast the day after I saw her, and that is now fourteen years ago. Looking at the softness and rapidity of growth I had very little hope. In another case a person with a large ulcerated cancer came to me; I took away the pectoral muscle and cleared the axilla. That patient is now living at the end of six years, without any sign of recurrence. I should therefore be inclined to go with the drift of this paper in urging early operation for cancer, looking much more hopefully upon such cases than we used to do, and believing that in many cases a cure so far as that particular cancer is concerned will be complete, though they may get developments of it afterwards. Here we are indebted partly to an improved knowledge of its pathology, and to a very large extent to those who have improved our surgical methods by the introduction of anæsthetics and antiseptics. The operation is now a mere bagatelle, scarcely involving any risk to the patient. We consequently do the operations very much earlier. I take it that is one of the explanations of our greater success now than in former days. We think more lightly of them, and in that lies our safety. Moreover, we may take comfort from the fact that the statistics which the author has brought before us apply equally in other departments of cancer—cancer of the tongue, of the lip, and in all places where cancer is visible and accessible. We remove early, and we get, on the whole, very excellent results.

Mr. NUNN.—Mr. Sheild has earned our best thanks for having so fairly and candidly placed before this society the reasons of the doubts and difficulties that he has experienced in dealing with cancer from a surgical point of view. And I think his maxim "operate early and operate widely" is pretty universally (with certain limitations) the policy adopted by all surgeons; at the same time that the force of the words "early and widely" may be variously accepted. Mr. Sheild, however, in speaking of operations, uses the terms complete and incomplete operations. The late Mr. Moore was, I think, the first to use the expression "inadequate operation" in his paper read in 1867 before this Society—it is this paper to which Mr. Sheild has made reference; it was published in the fiftieth volume of the Society's 'Transactions.' But can those who accept the term incomplete or inadequate say what is an adequate operation? The merciless logic of facts would seem to show that no operation is under all circumstances really adequate, and I would refer to Mr. Sheild's report of Halsted's cases and the results of this surgeon's operations. Mr. Sheild says where recurrence never takes place, such a rare

event is due to some change in the patient, and further that there must be something very remarkable about the cases where there is extreme latency; and herein lies the mystery of immunity and latency. He says one of the factors at work in such cases may be something in the body of the patient inimical to the rapid growth of the malady, and that the years of immunity enjoyed after even incomplete operations are owing either to the peculiarity of the growth or to the unsuitableness of tissue for its rapid propagation. It would be well if there were collated those cases of early and rapid recurrence where the disease breaks out with, so to speak, explosive violence, such cases as where the patient is dead within twelve months of the first discovery of anything being wrong with the mamma, where the whole breast has become solidified and the integuments covering the thorax are cuirassed and the upper extremity gorged with solid œdema. Such antithetical cases lead one to the belief that there must be something in the patient that conditions and underlies such a rapid propagation of the disease. That something is the fact that the cancerous disease may primarily attack the lymphatic system, glands, vessels, and lymph spaces. In such cases an adequate operation is beyond the range of possibility. Such cases are cases of lymph cancer in the sense that certain other cases are called duct cancer. I should hardly have ventured to trespass on your time had not Mr. Sheild pointedly made reference to Mr. Moore's paper of 1867. For in that paper the case numbered seven happens to be that of a woman on whom I had operated for cancer of breast in 1864, the tumour having been small and discovered in 1862. The woman was in November, 1866, readmitted under Mr. Moore's care with a tumour twice the size of an almond on the sternal side of my cicatrix; but, for the reasons he gave in his paper, he decided not to operate. It so happened that I was enabled to give the sequel to this case, for she died in 1870 in the hospital under my care, and the post-mortem showed that the recurrence or metastasis was in the liver and brain. I have published the case. These post-mortem appearances did not tally with the surmises which Mr. Moore published in his paper. Mr. Sheild has remarked on infrequency of cancer affecting the opposite breast to the one primarily attacked. I think he rather underestimated the frequency, for he says, "It is remarkable how seldom comparatively (referring to Group II) the second breast was affected." Dr. Coupland has published an analysis of 89 cases of mammary cancer examined after death, and he found cancer of both breasts in 19.1 per cent. ('Path. Soc. Trans.,' vol. xxvii, p. 264), *i.e.* about one-fifth—1 in 5 cases. Perhaps, however, Mr. Sheild may consider this a remarkable infrequency, from what he says in a sentence immediately following.

From my own observation about 8 per cent. will last from ten to twenty years, or about 1 in 13.

Cancer is a form of molecular degeneration followed by molecular necrosis or by gangrenous ulceration in mass; the surgeon steps in with the knife and rapidly and cleanly takes away the degenerating part. No justification by the theory that cancer commences as a purely local disease is therefore required, nor is there any reason for withholding operation because of cancer being due to a constitutional fault. I take this to be the moral of Mr. Sheild's able paper. In regard to metastasis being favoured or induced by complete operation, it is a point that requires exhaustive investigation.

Mr. HOWARD MARSH.—The great advantage of this debate is to be derived from the conclusions that we arrive at as to the kind of operation to be done. Undoubtedly operations have been very much improved of late years. I am one who always removes the whole breast, and I invariably open the axilla. I should like to ask Mr. Hutchinson whether he thinks that it is always easy or possible to feel enlarged glands in the axilla through the fat, &c. My own experience is—and I should have thought that we should all be agreed as to this—that it must be impossible in many cases to recognise an extensive disease in the axilla of fatty patients without opening it up. Seeing that this is quite a trivial procedure, I expected that we should all agree as to the necessity or at any rate as to the propriety of that part of the operation. There are four structures specially concerned in the original development of recurrence of cancer—breast itself, the skin, the fascia, with their lymphatic appurtenances and the lymphatic tracts in the axilla. These I think must be quite freely taken out. It may be a matter of opinion whether the breast should be completely removed or not, but I think the universal feeling is in favour of such removal. In the second place, skin ought obviously to be widely removed. There is no objection to this, because in aseptic wounds the skin can be brought together under great tension, and if necessary large flaps can be used as in Thiersch's method. As many recurrences take place in the skin it should obviously be widely removed. I suppose every one holds that the pectoral muscles should be dissected quite clean, and I think the axilla ought to be emptied. There seems to be some little uncertainty as to the difference between the complete and the incomplete operation. The only difference, I believe, with Halsted's operation is the removal of the whole of the pectoral muscle with the pectoralis minor as well. This is a point on which I differ. If there is to be recurrence after removal of the breast it must be in the skin or in the axilla. I have no knowledge myself of the recurrence of cancer in muscular tissue, therefore I do not believe that the removal of the pectoral muscle protects the



patient in any appreciable degree. The very extensive complete operation is dangerous *per se*. I have known people die from shock after it. I have watched surgeons sitting up with patients half the night to save them from dying of shock. In the next place there is no doubt that after the removal of both pectoral muscles the arm is often fixed to the side, the hand being in a state of permanent œdema with considerable pain.

Mr. BARKER.—My own experience as a hospital surgeon for the last twenty-three years has been tabulated completely in the list in my hand down to within a couple of years or so. Certainly for the last twelve years I have done the more complete operation,—that is to say, I have always recognised the necessity of stripping at any rate the pectoral muscle of its fascia most thoroughly, frequently taking away small portions of it and clearing out all fat from the axilla together with glands as high up as the clavicle in one mass with the breast, and as much skin as I could get away—compatible, that is, with closing or nearly closing the wound. That is, I suppose, a more extensive operation than was formerly done, but I hesitate to call it a complete operation. Even after these very wide-reaching operations we still get recurrence, either before or after the three years' limit. As to the relative risks of this as compared with the older operation, I do not think we have increased them by the innovation in the least. That is due to a great many factors, but judging from what I have seen and from my own cases there has been a great improvement and no increase in the mortality, while the vast majority heal *per primum* under one dressing and without drainage. There has been a marked increase in comfort to the patients, and consequently they have a shorter time in bed than of yore, with lessened risk of intercurrent troubles. I may mention that within the last fifteen years I have only lost one case (ten years ago), death being due to sepsis, and this might have followed a scratch. The paper has done great service in fixing our opinions from this point of view, and enabling us to get the opinion of older and more experienced surgeons, most of whom agree in saying that we get better results than formerly. That is something gained. Then, too, we know now that the operations are generally more complete. If you believe that they are not more dangerous we ought to be encouraged to adopt these more thorough operations. Sir Thomas Smith's statement as to the improvement in our results, immediate and remote, is difficult to reconcile with his suggestion that the removal of healthy glands may increase the risks of recurrence, because it is now the almost universal rule to remove them. How, then, are we to explain these improvements in the duration of life? I endorse what Mr. Marsh has said that it is absolutely impossible to detect the state of the glands

in the axilla through the skin. It is not always easy even when you have opened the axilla and hold them between your fingers. I would support Mr. Hutchinson's view in regard to the class of cases in which we sometimes gain the best results. We all of us see some of our best results in the apparently most hopeless cases. I heard to-day of a patient on whom I operated nine and a half years ago, clearing away the whole breast, the whole of the pectoral fascia, clearing out the axilla, which contained many small widely disseminated carcinomatous lymphatic glands. I accorded her at most six years' immunity, but she is still well.

The discussion was adjourned to February 22nd, on the motion of Mr. PEARCE GOULD, seconded by Dr. NORMAN MOORE.

PROCEEDINGS  
OF  
THE ROYAL  
MEDICAL AND CHIRURGICAL SOCIETY  
OF LONDON.

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*Tuesday, February 22nd, 1898.*

W. HOWSHIP DICKINSON, M.D., President, in the Chair.

NORMAN MOORE, M.D.,  
ROBERT WILLIAM PARKER, } Hon. Sees.

Present—34 Fellows and 7 visitors.

The minutes of the last meeting were read and signed.

The following recently elected Fellows signed the Obligation, and were admitted by the President:

Edwin Hurry Fenwick, F.R.C.S.Eng.  
John Alfred Gray, M.B.Lond., M.R.C.S.Eng.  
Arthur Corrie Keep, M.D., C.M.Edin., M.R.C.S.Eng.  
John Bowring Lawford, F.R.C.S.Eng.  
John McFadyean, M.B., C.M.Edin.  
Herbert Murray Ramsay, F.R.C.S.Edin.

The following candidates for the Fellowship were proposed for election, and their proposal papers were ordered to be suspended in the Library:

John Sydney Edkins, M.B.  
Thomas J. Horder, L.R.C.P., M.R.C.S.  
Llewellyn Caradoc Powell Phillips, M.B., B.C., F.R.C.S.  
Frederick William Robertson, M.B., B.S.

The following gifts were announced, and votes of thanks were awarded to the donors:

Syntagma medicum de morbis mulierum in libros iv distinctum a Nicolao Fontano, Amstelodami (1644): presented by Dr. John Abercrombie.



Diseases of Women: a clinical guide to their Diagnosis and Treatment; by G. Ernest Herman, M.B. (1898): presented by Messrs. Cassell & Co., Limited. Index-Catalogue of the Library of the Surgeon-General's Office, United States Army; second series, vol. ii (1897): presented by the Surgeon-General, United States Army. Twenty-fourth Annual Report of the Local Government Board; Supplement in continuation of the Report of the Medical Officer, 1894-5; Inland Sanitary Survey, 1893-5: presented by the Medical Officer of the Local Government Board.

The discussion on Mr. Marmaduke Sheild's paper on "Immunity and Latency after Operations for Reputed Cancer of the Breast" was reopened by Mr. PEARCE GOULD, followed by Mr. WATSON CHEYNE, Dr. NORMAN MOORE, Mr. WILLIAM H. BENNETT, Mr. FREDERICK TREVES; and Mr. SHEILD replied.

Mr. PEARCE GOULD.—It seems to me that we are met at the threshold of this discussion by one serious difficulty, in that we are no nearer a complete knowledge of the nature of the disease under discussion than we were twenty years ago. And we seem as far off as ever from any precise knowledge of the conditions which regulate the course and development of the disease. The discussion is rendered particularly difficult and embarrassing by the extraordinarily protean nature of cancer. It would be no exaggeration to say that there is no other disease which presents such remarkable variability in its clinical and pathological features as cancer, especially cancer of the breast. Cancer is a disease which may terminate life within a few months without any particular accident in its course, or which may extend over a period of even twenty-five or thirty years; it may for long years be limited to the organ in which it originates, or may spread in a few months to nearly all the organs and tissues of the body; when invading an organ or tissue secondarily it may creep with the slowest possible progress, or with a speed which almost makes its advance visible from day to day; it selects now one tissue or organ and now another to manifest its secondary growths. The primary growth is in one case massive, in another very contractile, in a third deeply ulcerating. In judging of the value of any therapeutical measure we are apt to think, or at any rate to speak, as if there was some degree of uniformity in the course of this disease. But nothing is further from the facts, there is no uniformity, and I have never seen any detailed description of the disease which would apply with precision to six consecutive cases. It is always difficult to arrive at sound conclusions from therapeutical observation, but this difficulty is enormously increased when, as in cancer, the natural course of the disease varies within extremely wide limits. It is from this point of

view that I wish to discuss Mr. Sheild's paper. In his paper Mr. Sheild has presented us with two tables of cases collected with great industry and care. In Table I are ranged cases in which there has been no return of the disease for a long period after operation for mammary cancer, and the patients have died from some other cause. In Table II we have a larger series of cases in which prolonged immunity from recurrence has, after many years, been followed by return of the disease either locally or in internal parts. Are we to regard all the cases in Table I as cured by the operation? Roughly speaking, we may; but evidently Table II has a considerable bearing upon our interpretation of Table I, and to be quite logical and scientifically accurate we are not justified in regarding any case as "cured" by operation until it has survived beyond the longest time at which recurrence has ever been met with, and unless all the organs and tissues, including all the bones, have been examined by a competent pathologist and found free from the disease. Many of the cases in Table I do not fulfil these conditions, but practically we may regard a patient as "cured" if she survives several years free from obvious cancerous growth, and dies from some other cause. The philosopher advised us to call no man happy until dead, and the surgeon should hesitate to pronounce anyone "cured" of cancer until he has died from some other cause. Coming now to Mr. Sheild's second table, we have to answer the question whether the long immunity from disease enjoyed in the cases here collected was the effect of the operation, and if so, how this effect was produced. In the first place we must remember that for every case of long immunity recorded in this table, every surgeon in this room could record many in which no such long immunity has been enjoyed; not only so, but the period of immunity after operation varies from a few weeks to many years, and cases are met with occupying every position between these two extremes. Lastly, when the disease runs its natural course in some cases secondary growths occur very speedily, but in others more slowly, even at an interval of many years from the first appearance of the primary growth. In the Cancer Department of the Middlesex Hospital we have special opportunities of studying the life-history of cancer, for we see the cases at the end of their course. With the help of the surgical registrar, Mr. Sidney Hulke, I have collected the cases of cancer of the breast that have ended their days here during the past ten years. I have limited myself to this period because at the last meeting Sir Thomas Smith suggested that possibly the type of the disease has altered of late years. There are 74 cases of which we have sufficient data. Of these, 26 had no operation, and we are able, therefore, to study the natural course of the disease. In 48 cases one or more operations had

been performed, and the natural course of the disease had been (or might have been) affected thereby. In the 26 cases in which no operation was performed, the average duration of life after the recognition of the disease was forty-five months; the shortest period was four months, and the longest was twelve years. In the 48 cases which submitted to operation the average duration of life after recognition of disease was 46·85 months; here, again, the shortest period was four months, and the longest period was twenty-five years—a case in which there was an interval of twenty-three years between the operation and the recurrence of the disease. These numbers are small, and the average is easily upset by a single very short or very long case. Looking at the figures more closely, I find that of those not operated upon 61·5 per cent. were dead within three years of the disease being noticed, and 50 per cent. of those operated upon: of those not operated upon 76 per cent. were dead within six years, and 89·5 per cent. of those operated upon. It would be wrong to lay down a law from these cases, but at any rate they ought to make us pause before assenting to the proposition that long immunity after operation is certainly the result of the operation: they illustrate the wide variation in the natural course of the disease, and the fact that this variation is as marked where operation is performed as where it is not.

With the aid of Dr. Voelcker, our pathologist, Mr. Hulke and I have taken out a table of the seat of the secondary growths in all the cases of cancer of the breast dying in the Middlesex Hospital during the last ten years. There are 113 cases of which we have sufficient data. There are 38 cases in which no operation had been performed, and 75 cases in which operation (one or more) had been made. Here is the table:

	38 cases <i>not</i> operated upon had secondary growths.			75 cases operated upon had secondary growths.		
	No.	Per cent.		No.	Per cent.	
Axillary glands . . .	31	83·6	...	42	56	...
Cervical glands . . .	13	34	...	17	24	...
Lungs and pleura . . .	27	71	...	44	57	...
Liver . . .	16	42	...	29	39	...
Bronchial glands . . .	8	21	...	9	12	...
Kidney . . .	7	18·5	...	13	17	...
Opposite breast . . .	7	18·5	...	7	9·5	...
Heart and pericardium . . .	4	10·5	...	11	14·5	...
Peritonæum and mesenteric glands . . .	5	13	...	14	18	...
Bones . . .	3	8	...	10	13	...
Brain and dura mater . . .	2	5·2	...	6	8	...
Spleen . . .	1	2·6	...	7	9·5	...
Pancreas . . .	2	5·2	...	3	4	...



	38 cases <i>not</i> operated upon had secondary growths.			75 cases operated upon had secondary growths.		
	No.	Per cent.		No.	Per cent.	
Uterus . . . . .	1	2·6	...	2	2·5	...
Ovary . . . . .	3	8	...	7	9	...
Supra-renal capsule . . . . .	3	8	...	2	2·5	...
Intestine . . . . .	1	2·6	...	1	1·3	...
Inguinal glands . . . . .	1	2·6	...	0	0	...
Iliac glands . . . . .	1	2·6	...	0	0	...
Thyroid body . . . . .	0	0	...	2	2·5	...
Local . . . . .	0	0	...	33	44	...

Among the 38 cases not operated on was one which had lasted nine years, and no secondary growths were found at the post-mortem examination. In 4 other cases the only secondary deposits found were in the axillary and cervical glands; 3 of these cases had lasted respectively four years and one month, three years and one month, and two years and nine months: in one case there were no clinical notes. Among the 75 cases which had been submitted to operation was one which had certainly lasted thirteen months, in which no secondary growth was found, and there were 10 other cases in which there was no visceral or internal growth:

Metastasis in supra-clavicular glands only . . . . .	1 case
„ axillary and supra-clavicular glands only . . . . .	3 cases
„ „ glands only . . . . .	2 „
„ scar and axillary glands only . . . . .	2 „
„ scar and axillary and supra-clavicular glands only . . . . .	1 case
„ scar and axillary and supra-clavicular glands and opposite breast . . . . .	1 „

In two of these cases the total known duration of the disease was two years and six months and three years and one month respectively.

In this table we have slight differences in the two groups of cases, differences which most probably find their explanation in the small number of cases dealt with. Allowing for this fact, and for the known great variation in the disease, these cases seem to show that operation makes no appreciable difference in the incidence of visceral and internal recurrence when the disease reappears, except only in the case of the axillary glands—an apparent and not a real exception, owing to the frequency with which these glands have been entirely removed by many operators during the last ten years. Looking at all these facts, sir, I would submit that it is only in accordance with the natural course of this disease that there should be long as well

as short periods of immunity after operation, and that the immunity enjoyed by the patients grouped in Mr. Sheild's second table is not necessarily to be attributed to the operation. There are many cases in which operation, for a time apparently successful, does nothing to affect the ultimate issue of the case. Another question that arises is whether the outbreak of the disease after a long interval of time is a fresh outbreak of the disease or a true secondary growth. One of the earlier speakers intimated that in some cases at any rate it must be regarded as a fresh outbreak. Obviously this introduces a new difficulty. With the exception of the cases of cancer arising in the opposite breast I regard all these cases of late recurrence as true secondary growths. The evidence in support of this view is, I think, abundant. It exists in the fact that there is no line to be drawn between early and late secondary growths, that these late growths occur in the same tissues, in the same regions, and in the same organs as the secondary growths of non-operated cases, that they often occur in tissues where primary scirrhus is never met with, and that they are identical in structure with the primary growth.

A study of cases which run their whole course without operation shows us that secondary growths in various parts may be postponed for a very long time, even years. It is not without significance, too, that in cases of recurrence, whether early or late, it is quite common to meet with several distinct recurrences manifesting themselves at the same time. This is quite contrary to the known history of primary malignant growths; they are single with very rare exceptions. We find these simultaneous recurrences not only occurring in the same tissue, but in different tissues and organs. Thus in one of my beds at the present moment is lying a woman whose left breast was removed for cancer in March, 1890. She remained well until February, 1897, when a growth appeared in her right femur, and in the autumn of 1897 growth appeared in the sternum, ribs, ilium, and skull. It is stated that local recurrences often occur in portions of breast tissue which have been left behind, and this is urged as a proof of their being new outbreaks rather than secondary growths. But the fact, if fact it is, surely in no way supports this view. These bits of gland tissue become diseased not because of their structure, but because they have been "infected" by the primary growth: the fact that these local recurrences are very much more frequent than recurrences in the opposite mamma is strongly in support of this view; and if further evidence is needed, it is to be found in the great frequency with which the disease returns *in loco* when the primary growth and only a limited portion of the gland around it is excised. We must apply the same term to both forms of recurrence, and it is contrary to all we know of cancer

to regard them as other than a secondary growth from "infection" from the primary focus.

One question still remains: what is it we effect by our operations? Do we influence the course of the disease by a traumatism? Do we by the removal of certain parts beneficially affect those left behind, and so enable them to resist the disease? Or do we simply and solely remove infected and infective tissues? If the last, our operation is simply an amputation, and as such can never seriously be spoken of as a "cure." We do not "cure" gangrene of the leg by amputation, any more than the lawyer "cures" a homicidal tendency by hanging the culprit. I think all the evidence points to the effects that are noticed being due solely to the removal of infective and infected tissues, and I hope that this word "cure" will not be used as freely in the future as in the past in speaking of operations for cancer. But when he attempts to remove these infected tissues the surgeon is met by serious difficulties, arising partly from the protean nature of the disease. The area of infection varies enormously in different cases, for both the rate of infection and the direction in which infection tends to spread differ in different cases. His senses cannot guide him, he can neither see nor feel the infected parts distinct from the non-infected. And yet if he is to accomplish his purpose he must anticipate internal infection, and extend his amputation beyond the area of local infection. He has no means of ascertaining either of these points, except by watching the issue of the case; time alone can assure him. The only distinction I would draw between operations is that of success or failure. The so-called "complete" operation is one observing certain anatomical limits. Now, apart from the fact that it is impossible in any given case to assert that the particular anatomical design has been actually accomplished, we have to admit that even if perfectly performed it may fail to remove all the locally infected parts (it may also remove parts that are not infected), and it may be too late to anticipate internal infection. We never know whether a given operation is "complete" or not; and even if it is, it may fail in its object. On the other hand, under certain conditions — unrecognisable, unfortunately — a less extensive operation may remove all the infected tissues, and also anticipate internal infection, and so be truly successful in its issue. Our operations must be early and wide, and the earlier and wider they are the greater the success that will follow them. But the true distinction to be drawn between one operation and another is the success or failure that attends it, and I would wait until the final issue of the case was known before labelling it at all, and then call it "successful" or "unsuccessful." "Adequate" is a better term than "complete," but as we can only judge of adequacy by success I prefer the latter word. If we bear



in mind the great variations observed in the life-history of cancer, we find at once an explanation of the very varied results of operations apparently identical, and of similar effects of varying operations, and the solution of the difficulties presented by striking cases, such as some of those cited by Sir Thomas Smith. Mr. Heath has pointed out that in some of the recently reported statistics of the so-called "complete" operation there is an undue proportion of cases of recurrence in internal organs, and he suggested that a more extensive local operation might possibly tend to concentrate the disease upon the viscera. I regard this fact as one of the strongest arguments in support of this form of operation, for whether an operation prevents or fails to prevent internal recurrence depends upon the time at which it is performed, and not upon its local extent. The only claim for the complete operation is that it removes the whole locally infected area in a larger proportion of cases than does the older operation, and therefore prevents local recurrence. The fact pointed out by Mr. Heath shows that this claim is justified. From another point of view the fact is not so unsatisfactory as it might seem, for when we compare the rapid and often but little painful course of visceral cancer with the long-drawn-out agony of the local disease, we cannot fail to welcome any measure that spares some of our patients the latter, even if it leaves them still exposed to the former evil.

Mr. WATSON CHEYNE.—I regret that I was unable to be present at the former meetings, but I gather from the notices in the journals that the discussion chiefly turned on the relative merits of the old and the new operations for breast cancer. I do not propose to enter at length into that question to-night, as I already discussed it fully in the Lettsomian Lectures two years ago, and nothing has since occurred to lead me to alter the views I then expressed. And, further, I intend at the end of this year, when three years will have elapsed since the whole 61 cases there referred to were operated on, to put together and publish the results then found. Even two years ago I was able to refer to 21 cases in which more than three years had elapsed since the Heidenhain-Stiles operation, and of these 12 had had no further disease; and had I gone back two months into 1889 I could have added another case, probably the first in which I carried out the Heidenhain-Stiles operation, which also remains well to this day, making 22 cases with 13 successes. And I have good reason for believing that at the end of the year the proportion of successes will be found to fall not very far short of the above. When I wrote the Lettsomian Lectures I looked out all the statistics of the old operation which I could find, and the results showed that on an average 10 per cent., and in some cases up to 18 per cent., remained well after three years. Hence it is not at all surprising that eminent surgeons of large

experience are able to come here and narrate a number of cases in which prolonged survival had occurred, but so far as I can see no statistical results were given of the total number operated on and the proportion thus remaining well. Unless put in statistical form, however, these cases, interesting though they may be in themselves, are of no value as an argument against the Heidenhain-Stiles operation; if so used they sink to the level of impressions and not actual statistical facts, and we all know how erroneous impressions are apt to prove when submitted to statistical study.

Mr. Sheild has compiled two tables in his Appendix, the first being with the view of showing "that the results of the so-called incomplete operations are not so universally bad as is generally estimated." This depends on what is meant by the term "universally bad," and Mr. Sheild does not define it by percentage statistics or otherwise. I should not call a percentage of recoveries of 10 per cent. "universally bad," considering the nature of the disease we have to deal with and the operation performed, and I do not see in what way this table alters that average. It is made up of cases from a number of practices, and the total number of cases comes to 44. I should have expected more cases from these practices on the 10 per cent. average. The table merely emphasises the fact, which has not been disputed, at least I have never disputed it, that even with the old operation a certain number of cases remain free subsequently. Although one now advocates the entire removal of what I may term the primary lymphatic area in every case of breast cancer by the Heidenhain-Stiles operation<sup>1</sup> on account of the irregular and frequently wide distribution of the cancer cells over that area, one does not imply that such wide and irregular distribution occurs in every case. In some instances the cancer cells apparently follow a straight course to the glands, or have not even reached the glands when the operation is performed; and it may quite well happen, and it does happen, that in some of the old operations all the infected area is removed. Reference has also been made to cases which have remained well even after removal of the tumour and a little of the surrounding tissue alone. Here, again, it is just a fortunate chance, and surgeons have long ago come to the conclusion that many more successes will be obtained by removal of the greater part of the breast than by removal of the tumour alone. The difference between the two procedures is, however, only one of degree and not of kind; in both breast tissue is left

<sup>1</sup> The new operation does not mean removal of the pectoral muscle. It is the entire removal of the breast, wide removal of skin, pectoral fascia, and all fat and glands in the axilla. Removal of the pectoral muscle is but rarely necessary.

behind, but where more rather than less is taken away the success is correspondingly greater. Heidenhain and Stiles practically say, if you will go a step further and remove the whole breast and the primary lymphatic area you will have a still greater certainty of getting rid of the disease, and increasing experience in various countries shows that this contention is correct. The whole question put shortly is this: are we to be content with 10 to 18 per cent. of success, which is the best that can be got by the old operation, or should we not do a more extensive operation with the view of obtaining a success of 40 to 50 per cent., which increasing experience shows can be got by the new operation? I freely admit that some of my cases, say 3 or 4 out of about 20 successes which I now have, would, according to the statistical evidence, have been free from the disease had I performed a less extensive operation than I did. But which 3 or 4? I cannot tell. The size of the tumour is not a criterion, nor is its situation, nor even its rapidity of growth. Had I attempted to select cases in which to perform the old operation I should probably have chosen the wrong ones, and then these 3 or 4 cases would not now be well. But further, as I shall immediately point out in speaking of Mr. Shield's second group, these 3 or 4 cases would also, even though properly selected, still be exposed to a risk of reappearance of disease in breast tissue left behind which they do not now run. And, after all, what have these 3 or 4 patients lost by having had a more extensive operation than was absolutely necessary performed on them? At the very most a slight restriction in the upward movement of the arm, a small penalty to pay for increased certainty. As a matter of fact I have seldom found that a patient cannot do her back hair after this operation, and if a woman can do her back hair she is generally well satisfied. The second table has reference to possible latency of cancer cells after an operation, and to the time limit when one may presume that a patient is free from disease. There are three possible interpretations of these cases of supposed latency, and one must be quite clear on that matter. Some of them may be really cases of dormancy of cancer cells waking to life again under conditions which we cannot gauge; some may be instances of a new outbreak of cancer altogether, and some may simply mean very slow growth of the cancer cells. On looking at this table more closely we find that the cases fall readily into two groups,—1, those where recurrence took place in the region of the scar; and 2, a much smaller group, at the most 20 in number, where recurrence took place in parts away from the scar, such as glands, &c. Now as regards the first group we know that in these instances breast tissue was left behind at the time of the operation, and we know also that the great majority of these locally recurrent



nodules occur in connection with breast tissue (one can see acini and breast lobules in the sections), and these cases are therefore not, to my mind, convincing proofs of true latency. They may quite well be fresh outbreaks of disease in the breast lobules left behind. Against this supposition Mr. Sheild very cogently remarks that it is curious, if it is fresh disease, that it does not appear more frequently in the other breast. In reply to this I would point out how comparatively rarely the second breast becomes attacked in cases not operated on or in cases which recur after operation. Why did the disease begin on one side and not on the other in the first instance? Surely because there was some local condition existent on that side and not on the other beforehand. And if this condition existed at first, why should it not continue to exist in the parts of the breast left behind? I do not deny that some of the cases in this group may be instances of slow growth or of real latency, but as the other interpretation is possible I do not think they can be utilised as proof. In passing I should like to point out that, whichever interpretation is adopted, these cases form a strong argument in favour of the Heidenhain-Stiles operation. Had it been performed the probability is that these patients would have remained permanently well, for the tissue in which recurrence took place would have been removed. The second group is really the important one, and though these cases cannot be regarded as examples of fresh outbreaks of disease, they do not all imply dormancy of cancer cells; some may be merely examples of slow growth. As regards dormancy, while one must admit the possibility, we have really very little evidence. The best example is given by the results of Dr. Beetsen's oöphorectomy cases. In his first case the cancer entirely disappeared to the eye and touch, and remained absent for two and a half years, but he tells me that one or two nodules have again appeared. Here the cancer cells must have atrophied in large numbers, but some retained their vitality, lay dormant for a time, and then again sprang into life. I have also had a somewhat similar experience. In my first case remarkable improvement followed oöphorectomy, and for six months the cancer steadily dwindled, but at the end of that time growth recommenced. But though we must admit this possible dormancy it is an extremely rare occurrence, and Mr. Sheild has only got twenty examples from many sources, and some of these are by no means certain, and may merely be cases of continued though slow growth, as in atrophic cancer. As regards this group also I may point out that some would not have recurred had the Heidenhain-Stiles operation been done, for the glands containing these cancer cells would have been taken away. In reference to the value of the three-year limit, König, who gives the worst account, says that 15 per cent. recur after three years.

He is, however, speaking of the old operation, where, as I have just pointed out, tissues are left behind which may lead to new growth or contain cancer cells. In my own experience I can say that no case which has passed the three-year limit without fresh disease after the Heidenhain-Stiles operation has relapsed subsequently, and I have several of seven and one well into nine years' standing. Hence, although I see nothing magical in the period of three years, all experience confirms the view that the chances of recurrence are comparatively slight in patients who have passed that period after the Heidenhain-Stiles operation. I would put it in this way. If in the first instance I had to do with a fairly large and pretty rapidly growing cancer, *i.e.* one where recurrence would take place pretty soon, and three years elapsed without fresh trouble, I should feel pretty confident that the disease had been entirely eradicated; while if it had been a case of the type of atrophic cancer I should still hesitate to pronounce the patient cured till a longer time had elapsed, for cancer cells might be growing slowly beyond the area of my operation. There are still two points to which I should like to refer very shortly. The first is that both Mr. Sheild and Sir Thomas Smith have referred rather pointedly, in connection with the Heidenhain-Stiles operation, to the frequency with which metastatic deposits are mentioned as the cause of death. But I think I am right in saying that at least 90 per cent., if not, indeed, all of those who die of cancer of the breast, die not only of the local recurrence, but also of metastatic deposits, only in many cases the local disease forms the most noticeable feature, and is specially referred to in the records. But after the Heidenhain-Stiles operation external recurrences have become much rarer, and in the majority of cases there is practically nothing left for the patient to die of but the metastatic deposits; and further, in the absence of external recurrences, the patients live longer, and the internal deposits have time to grow and to become more noticeable. I do not think there is more in it than that. Indeed, as formerly 80 to 90 per cent. of the patients operated on died of cancer, whereas now only about 50 per cent. die of cancer, the frequency of metastatic deposits is much less than after the old operation. It is to me surprising, after Prof. Goldmann's recent work, that we do not lose more patients from internal disease than we do, for he has shown that from a very early period the walls of the veins become attacked by the cancerous growth, and he attributes some of the local distribution also to spread by the blood-vessels. His researches show in what imminent danger of metastasis his patient is from quite an early period of the disease. The second point is as to the nomenclature of the operation. I notice that several surgeons speak of this operation as "Halsted's." This, I think, is very

unfair to the authors of the operation. If an engineer builds a bridge we call it by his name, even though he may never have laid a brick; we do not call it by the name of one of the masons. In this case Heidenhain and Stiles have not only described the pathology and mode of spread of breast cancer, but have told us exactly what must be done as regards operation in full detail, and it should therefore be called the Heidenhain-Stiles operation if the name of any individual is to be given to it. If, however, you insist on giving the artificer's name to it, be sure to take the one who has priority, and not the one whose paper happened to attract your attention. Heidenhain and Stiles have both put their views into force, and long before Halsted wrote the operation was being done in Germany, in Griefswald and other places. I myself published a full account of it with cases remaining well after three years in the 'British Medical Journal' months before Halsted. As I have said before, however, if a name is to be given to it, it should be called the Heidenhain-Stiles operation, after the men to whom the whole credit of it is due.

Dr. NORMAN MOORE.—It appears to me that the pure pathological aspect of the question has been too much neglected in this discussion. I have heard such expressions as "prone to another attack," "a second attack of cancer," and so on. Now how many examples can any one here produce of a second attack of cancer? I have never seen post mortem what I should consider as a second attack of cancer. I have never seen a patient whose breast has been removed some time before die of an entirely different cancer, say a cancer of the pylorus. I have never heard of such a case in my own experience. If that be so it has an important bearing on this question. If cancer originates—and in this discussion the speakers seem to have lost sight of Waldeyer's view that cancer arises from some special epithelium,—if it be true that in no case is a second epithelium attacked, the whole inquiry resolves itself into a question how far the disease has proceeded from the original focus. There are two ways, of course, in which it may proceed from that original focus. It may grow directly, and those in favour of an extensive operation would say that they remove all the local growth. The other form of extension is that further into the body, the old metastasis, a word of which many people have not considered the true nature; I think it would be much better unused. What is it? It is merely the point at which some cells proceeding from the original growth have stopped. If the cells have started, if they have got into the chest or body, extensive as may be the operation, they still reappear. All surgeons have spoken pretty clearly in that direction, they have not given us the slightest grounds for knowing in any particular case of cancer of the breast whether



there is any internal growth at the time of the operation, or whether the cells have started on the formation of such a growth. Is this not so? It seems to me that the only way we can proceed is by the accumulation of pathological knowledge. We do know something about it—about new growths in the viscera, for example. We know where the new growths are likely to be found; we know that most internal growths, after the lymphatics, in the neighbourhood will be in the liver or lungs; but in particular growths, as in a scirrhus, there will be a particular liability of the kidney or heart, which are likely to be infected by secondary growths rather than the liver or the lung. It is by the accumulation of that kind of knowledge, and by the careful distinction of the numerous species of carcinoma of the breast, that the second part of this question may be determined.

MR. WILLIAM H. BENNETT.—It is, I fear, impossible in a prolonged discussion like the present to avoid considerable repetition in the views expressed by successive speakers; at the same time I think it must be allowed that the subject is of sufficient importance to justify those who have definite views in relation to it, in taking the opportunity for expressing their opinions. Not the least important of the questions arising is that which refers to the application of the word cure as used in describing the results following upon operations for cancer generally, but in the present discussion limited to cancer of the breast. Are we or are we not justified in applying the term “cured” in cases of removal of the breast for carcinoma if the disease does not recur earlier than three years from the time of operation? The common experience of all surgeons is, I suppose, that upon advising an operation in these cases the patient immediately asks whether the treatment will effect a cure, and there is no doubt that the patient’s interpretation of the meaning of the word is permanent relief from the disease. Now it is perfectly certain that we cannot in any case of cancer promise what the patient understands by the term “cure.” I cannot, therefore, escape from the conviction that if we are to use deliberately the term in relation to a three years’ immunity from disease, we shall forfeit, I think deservedly, a great deal of that reputation for candour and honesty which we now possess. It has been stated, I think by more than one speaker, that a cancer, if it does not recur for a considerable period after removal, may be considered to be cured with as much reason as many other diseases,—for example, pneumonia, in which one attack may be followed at any time under certain circumstances by another of the same kind. I hope I may not be considered more than usually obtuse when I say that I cannot see any analogy between an acute disease tending naturally under

favorable circumstances to recovery, and a disease like carcinoma, which, so far as I am aware, has never been known to show any inclination to recovery in the sense of that shown by these other diseases; and I confess I have been unable to conceive any line of reasoning which would justify a comparison between the occurrence of a second attack, say of pneumonia, and the recurrence of carcinoma about the scar of an old operation wound, no matter how long that recurrence may be delayed. I also understood Mr. Hutchinson to say that the occurrence of cancer at long periods after removal may be regarded not as recurrences, but as developments of the disease *de novo*, and he quoted as an example the occurrence of cancer in the remaining breast long after one had been removed for the same disease. In the example mentioned it is conceivable that the second cancer may arise *de novo*; but what is commonly met with by surgeons generally as a recurrence is an occurrence of the disease in parts in the immediate neighbourhood of the original growth, or in parts in direct lymphatic connection with it. In such cases, at all events, it is difficult to see the justification for the belief that the later appearances are due to subsequent development of cancer *de novo*. Mr. Sheild has spoken of the vagaries of cancer, and no doubt in some cases of cancer of the breast the history after operation justifies the term. One of the first considerable operations I performed in private practice was the removal of a scirrhus of the mamma. Before coming to me the patient had consulted an eminent surgeon of the day, who very properly declined to operate unless he were allowed to remove the whole breast. To this the patient would not consent, and I was persuaded to undertake the removal of the obvious tumour, leaving the remainder of the breast. The tumour proved upon microscopic examination to be a rapidly growing carcinoma. That operation was performed in 1878; there has been no recurrence; the patient was perfectly well two years ago, and to the best of my knowledge is so still. Six years ago I removed from a patient at the same operation both breasts for scirrhus; the patient died with extensive recurrence in less than a year. A few months later I removed both breasts in another patient, first on one side and a few weeks afterwards on the other; in that case there has been no recurrence of any kind, and the patient was a short time since in perfect health. In both of these cases the tumours were microscopically identical; the patient who succumbed quickly had the stronger physique of the two.

The occurrence of cases of this kind—and many such could probably be described by any surgeon of considerable experience—points very strongly to the fact indicated by Mr. Sheild that the growth and recurrence of these cancers, however local the neoplasm may originally be, are strongly influenced by the

idiosyncrasy of the individual in which they occur, some individuals providing a soil which favours the development of the disease rapidly, whilst in others the soil is such that the growth of the disease may be so slow as to be hardly perceptible. In the same way there can be no doubt that some patients are less able to resist the recurrence of the disease than others. It is necessary in estimating the value of operations not to lose sight of these points, because there is no doubt in my mind that the long immunity which sometimes follows operation is often due rather to the idiosyncrasy of the individual than to the nature of the operation—a point which should to some extent, at all events, influence our decision about the propriety of the use of operations of extreme severity as a routine practice. The surgeons who a few years ago performed the old operation were, I believe, too pessimistic in their views; the surgeons of to-day, who are wedded to the routine practice of very extensive operations, err, I think, in the opposite direction, and are inclined to claim too much for their methods: at all events, it is clear that the old, comparatively slight, operation was not altogether infrequently followed by results which could not be excelled by those of the new operation. For my own part, I believe an operation midway between the old and the very new is, in the present state of our knowledge, the rational method to adopt. By this I mean that in all cases of removal of the breast for carcinoma the pectoral fascia should be carefully cleaned away and the axilla cleared out, because I do not believe it is possible to be in any way sure that the axillary tissues are free from disease until they are actually dissected out. Moreover the extension of the operation for the clearing of the axilla does not materially add to its severity. With regard to the pectoral muscle, if it is in any way involved in the disease it should be removed, but the removal of the muscle in an early case of carcinoma in which the mass is freely movable and the muscle free I regard as unnecessary mutilation. I cannot go so far as to say with Mr. Howard Marsh that recurrence of carcinoma in muscle is unknown to me; it must, however, be very rare. I have seen one undoubted case of isolated recurrence in the substance of the pectoral muscle three years and four months after the removal of the mamma on account of a small carcinoma near the nipple. Before we advocate the universal adoption of the very extensive operations in cancer of the breast, I think we require more information than we at present possess to show that the increased severity of the proceeding ensures a proportionate benefit for the patient; and before any reliable judgment can be formed upon this point I venture to submit that what we especially want is information as to the immediate effect upon life of some of these very extensive operations, *e. g.* such an operation as that which



includes extensive removal of the skin of the thorax, the entire clearing out of the axilla and the removal of both pectoral muscles. This information we require not only from one or two of those who practise the extreme method, but from the generality of those surgeons who have accepted the extreme method as a routine practice, for I need hardly say that the value of a treatment is to be estimated not upon the results achieved by one or two of its exponents, but upon the results attained by the generality of those who practise it.

Mr. F. TREVES.—It seems to me that the time is hardly yet ripe for the forming of a decided opinion upon this question. If any intelligent layman had listened to this discussion, I think he would have formed the impression that it was carried on by men who had not sufficiently complete evidence whereupon to base a true opinion. We have had many conflicting views presented, and there are some grounds for the belief that we do not know a very great deal of the subject of which we are talking. It is true that the morbid anatomy of cancer is as complete as it probably ever can be, but I question whether our knowledge of the pathology of cancer has advanced to any appreciable extent during the present century. The mystery of the geographical distribution of cancer, the peculiar recurrences in some cases and the immunity in others, are still unexplained. I have seen the red secondary nodules of cancer which often form in the skin vanish and return, and readjust themselves in a manner impossible to explain. The effect of certain infections upon cancer is quite unexplained. The effect of the removal of both ovaries upon some cancers is equally inexplicable. In one case under my care, after the third failure of operation to relieve a cancer of the breast, removal of the ovaries was followed by disappearance of the mass of cancer left behind, and the woman is now in apparently good health. These facts make one ask whether we are right in assuming that the proper treatment of cancer is by excision. That has not yet been proved. It appears at the present time to be the best available measure, but that it is the right treatment is not yet clear. The evidence that has been adduced has not placed this "new" operation in an entirely brilliant light, nor has it justified the contempt with which the old operation has been alluded to. Is the new operation a comparatively harmless one? We know and hear of deaths from it, and even if the patients do not die, is it without mutilation and inconvenience, and can we give the assurance that if they are free for three years they can be considered as cured? I would only allude to two cases I have seen which illustrate a phase of the new operation. The patients in question, whom I saw within twelve months of the performance of the new operation, were in a deplorable and hopeless condition. In both there was recurrence; in both the arm was firmly bound down

to the side; in both there was eczema in the axilla; in both there was œdema of the limb, and in one there was intense neuralgia, while in the other there was some paralysis; in both the limb was absolutely useless. I venture to think that in both these cases the operation had added materially to the patients' sufferings.

Mr. SHEILD (in reply).—It often happens that a paper originates subjects for debate which were little intended by the author, and this has markedly happened upon the present occasion. My object was merely to bring out the experience of those who have been long in surgical practice as to the ultimate results of operation upon cancer of the breast. I must say the favorable cases have surprised me, and I expect many others. I feel with Mr. Treves that the time is hardly ripe for a discussion as to the comparative value of operations. I distinctly stated in my paper that I could have collected many more cases; and if proof were needed of this, it would be found in the remarks of Mr. Heath, Mr. Barwell, Mr. Hutchinson, and others. All the surgeons were able to adduce a number of these remarkable cases, even up to the limits of twenty years and more, so that the criticism of Mr. Cheyne as to the paucity of the cases I have collected is scarcely opportune. To follow each speech in detail would be a long and tedious matter. The mass of material is very large, and the interests of the Society will be best furthered by my considering the subject as a whole, particularly drawing conclusions which seem warranted, and which may serve to increase our knowledge. In the first place, it is obvious that certain terms must be clearly defined. By "latency" I denote "lying hidden;" by "immunity" I mean "freedom from," the ordinary English acceptance of the word. "Cure" I will postpone discussing until the end. For the terms "complete" and "incomplete" operation I would substitute lesser and greater, as not liable to offend anyone. It is clear that the majority of the speakers rather mixed together many ideas of operating, so that the results they give must be taken in a very general sense. The type of the lesser operation is the removal of the breast, but not of necessity its outlying lobules, or the fascia, or the glands in the axilla. The type of the greater operation would be the removal of the mamma and its prolongations, the fascia, glands, and pectoral muscles. Now speakers have inclined in their remarks towards one group or another. Sir T. Smith and Mr. Teale are examples of those who have practised the lesser type operation, and Mr. Cheyne is an example of an exponent of operations of the greater type. Mr. Heath, writing many years ago, seems to have advocated and practised a very thorough operation, and the statement of Mr. Cheyne that in the old operation the breast was never removed is far too absolute. We may draw a clear conclusion

that operations of the greater type, especially of the extremes of the greater type, have not taken root in London among the majority of the leading surgeons. Indeed, Mr. Treves emphatically condemns them, and speaks in no uncertain terms of the miserable condition of some of these patients after operations of magnitude. Here I may shortly state my belief that no one type of operation can be performed in all cases of a disease of such infinite variety as cancer of the breast. There is one kind of cancer which I am surprised no one has alluded to, cancer of a peripheral lobule of the mamma, which may, in exceptional cases of it, safely be removed, leaving the mamma. If my memory serves me, a remarkable case of this nature was reported by Mr. Gould, when three years after operation the disease returned in the glands, not in the mamma. So when the disease is fixed to the pectoral and fascia, and great glandular implication exists, I can conceive it being justifiable in a strong patient to remove the muscles, but such extreme operations are rarely to be done in the weak, bronchitic, or fat and intemperate. I am an advocate for removing the sternal part of the pectoral only in cases of great glandular contamination, since it gives such free access to the axilla, and increases but little the hæmorrhage or the time of the operation. Many of the older surgeons who have spoken seem even now disinclined to explore the axilla for enlarged glands. Personally I feel very sure about this. It is impossible to feel the glands without incision, and we must not fall into the error of thinking that the only evidence of cancerous disease is what we can see or feel. I have seen sections of axillary fat and muscle filled with showers of infective cells, and I believe that those who do not clear the axilla, often shut up flaps leaving undetected masses of cancer behind. Recurrence in such instances is a clear misnomer.

As regards the comparative dangers of the types of operation, Mr. Cheyne, Halsted, and others, have shown that with asepsis the mortality is slight. It stands to reason, however, that the risk must increase, *pari passu*, with the magnitude of the operation. Personally I lay great stress in most cases not only on asepsis, but rapidity of operating, and especially the avoidance of "drenching" the patient with ether through a long operation. Mr. Hutchinson spoke of the now trivial nature of breast operations, and I was rather sorry he did so, because these and similar cases are only safe if done with elaborate care and circumspection. I hope my words will travel beyond this room when I state that these and similar operations are now done in the most improper manner, by those who lack the knowledge and experience painfully and laboriously culled by surgeons after years of hospital work. I also wish to add my support to Sir T. Smith when he stated that it needed a robust faith in



the local origin of cancer to amputate the arm in cases of cancer of the breast. Indeed, such operations as this, division of the clavicle, dissecting out cervical glands, and so on, carry their condemnation in the results of the performance of them, and can seldom come within the scope of legitimate surgery.

Mr. Treves sounded a warning note of speaking dogmatically concerning a disease of which we know so little as cancer, and this is a criticism which must apply to several of the speakers when they seemed to assume that such and such results were the direct outcome of their special method of operation. Here the remarks of Mr. Gould are pregnant with meaning, when he showed by cases extracted from the Middlesex Hospital records how narrow a division there was between cases operated upon and those untouched as regards ultimate results. We must not confound the results of operations with the pathological eccentricities of an ill-understood disease.

Much difference of opinion has been manifest as to when returns of the disease are true recurrences or fresh manifestations in a predisposed individual. Here, again, both theories are tenable, and it is unwise to be too dogmatic. A cancer of the liver or ovary, occurring say ten years after the removal of a scirrhus breast, may be a fresh outbreak. This view was upheld by Mr. Butlin and Mr. Hutchinson, and it behoves one to differ with caution from the opinion of two past presidents of the Pathological Society of London. Yet there are these cogent and powerful arguments against them. A large proportion of returns of the disease are in the neighbouring glands or in the bones. Who has seen primary cancer of bone? How rare is primary cancer of a lymphatic gland! Again, some 40 per cent. of returns—I quote from memory from Dr. Coupland's researches—are in the pleura, lung, or liver, structures in lymphatic contiguity with the primarily affected area. All this makes me believe that these manifestations are true recurrences, and this affords the strongest argument for early removal, which indeed Mr. Hutchinson, notwithstanding his support of the "fresh manifestation" theory, strongly advocated.

Lastly, I come to speak of the matter of cure of cancer. In this great differences of opinion are manifest. Mr. Cheyne has defended the use of the term with a warmth and vigour worthy of a better cause. It is unfortunate that our profession is apt to select words from the English language and pervert their ordinary meaning, whereby much confusion is introduced, and medical language is apt to become hazy and nebulous. Mr. Butlin was anxious that this term should be employed because it gave encouragement to our patients and to the public. We all know what a common-sense Englishman means by the term cure,—the eradication of a disease or the subduing of it, leaving the organism in a healthy and vigorous state for an indefinite

period. As an example of what I would mean as the cure of a tumour I would take the extirpation of a lipoma. We can, without speaking of or using the word in a "modified sense," or writing it in inverted commas, tell such a patient that he is cured. And confusion must not be caused by supposing that there are kinds of cancer not curable. An epithelioma growing on the scar of an old burn, for instance, may be generally definitely cured. It never returns. The patient is left strong and well. Can we say this in cancer of the breast? Let us suppose that we accept the three-year limit, and pronounce our patients "cured" if they pass this time without recurrence. This is copied in the text-books and generally accepted. The wife of a clever educated man gets cancer of the breast. She is operated upon and remains well for three years. Her husband doubtless reads the text-books, and not noticing the inverted commas round the word cure, he says, "All is now well!" "The disease is now conquered!" But suppose that in four years, as in the case mentioned by Mr. Heath, nodules form in the scar, and the patient dies of cancer in the pleura or lung; I ask this meeting, what would such a man think of our profession? Would he not as likely take his wife to the first quack or herbalist he could meet with as to a member of the profession which have so cruelly deceived him? Prognosis in disease is always difficult. In disease we do not understand it is little better than rough guess-work. In two bad maladies especially, phthisis and cancer, the patients have sometimes lived long enough to witness the funerals of the doctors who had years before condemned them to a speedy dissolution.

Mr. Butlin, in a criticism on my paper, complained that it would be taken in a pessimistic sense. Indeed, I hold quite the contrary view. Granted that many of the extraordinary cures which have come to light in this debate are exceptional, there are enough of them to show us and to show the public that the results of operations are far more favorable than is often thought. We may, indeed, with truth and propriety urge these considerations upon them, and tell them of the years of freedom that sometimes occur, of the excellent results of operating upon recurrences. Thus encouragement is given where so much is needed, and we do not commit ourselves to definite statements, so likely to be ultimately falsified, and so likely therefore to shake the faith of the public in legitimate practice. It does not follow, because the operations of past years are able to produce excellent results, that the more extensive operations may not produce still better. And in years to come, those of us who are able to attend a similar debate in this room may hear still better results in the treatment of a very distressing malady.

